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AUTHOR Venezky, Richard L.
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ABSTRACT

The difficulties involved in putting reading theory into practice are summarized in this report, which provides examples of the fragmentation of research by many theoretical models that inhibits their practical application. Throughout the discussion, references and evaluative comments are made concerning the papers that were presented at the Pittsburgh conference on the theory and practice of beginning reading. The paper concludes by urging researchers to refocus their attention on the school, the classroom, and the teacher, defining problems at those levels and working out solutions to them in the laboratory. Discussion following presentation of the paper is included. (RL)

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Summary of the Second Pittsburgh Conference on Theory and Practice in Beginning Reading

Richard L. Venezky
University of Delaware
Department of Educational Foundations

This paper was presented at the conference on Theory and Practice of Beginning Reading Instruction, University of Pittsburgh, Learning Research and Development Center, May 1976.

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The task of pulling together the ten papers and the various incidental bits of music that occurred in the last two days, especially under the aegis of a theory-practice relationship, seems to me to rank in difficulty somewhere between catching tigers in the desert and cleaning the Aegean stables.

Most of you probably know that some very good theoretical approaches exist for catching tigers in the desert. There has been some problem putting these into the practice, however, which I think reflects many of the problems we have here.

For example, one good approach is to take a cage just about large enough to get a tiger into and place it right in the middle of the desert. You stand inside the cage and then do an inversion of axes. That puts the tiger in the cage and you outside.

The second method that I recall--and I believe there are ten basic methods for catching tigers in the desert--is to take a large sieve and sieve all of the sand. When you are through, what's left in the sieve is the tiger.

Since we can't listen to Mozart's clarinet concerto, although we were teased with it by Dr. Fletcher yesterday, I thought I would organize my remarks in a similar fashion to that particular work.

There will be an opening movement, an andante, dealing with theory; a somewhat slower movement, a largo, although not totally lugubrious, dealing with practice; and finally a very quick presto, dealing with the relationship between the two.

When the baton is put down, I'll add some didactic comments, similar to the way Leonard Bernstein does when he gets through with Peter and the Wolf and starts fiddling around with the bassoon and the various other instruments, to show you which animals are played by what. This will be a short coda where I will make very foolish remarks about where we should all go from here, other than to the airport.

In talking about theory, I want to touch on what theory is, what theories we have now that are relevant to reading, and especially to reading for compensatory education children, and then to discuss the difficulties in dealing with theories in relation to reading.

The literature on the epistemology of science, especially that by Carl Popper or Abraham Kaplan, portrays modern experimental science as progressing from observation through cycles of hypotheses and experiments, until a theory emerges that predicts fairly well the observable phenomena in question, but furthermore is a theory that is no longer challenged by its opponents. A true theory must not only account for observable phenomena, it must also have wide acceptance.

The field of reading has never been at a loss for hypotheses or theories. In the beginning there was, for example, Javal's rhythmic eye movement theory that emerged in the late 1870's. Javal suspected from observation that eye movements during reading were not continuous, but occurred in jumps or saccades. Javal concluded, furthermore, that these movements were rhythmic.

This theory was fairly well accepted for about 20 or 30 years, but with the introduction of the corneal reflection technique for measuring eye movements, data emerged that was not compatible with the theory. Yet, surprisingly, for some researchers today, the rhythmic eye movement theory has not been satisfactorily refuted. So a theory is something that accounts for observations, meaning usually in an economical way, and is generally accepted at least for the time being.

Related to a theory is a model which is a term which is used quite loosely by pretty much everyone today, perhaps as loosely as the term system (I flew into Pittsburgh on the Allegheny Air System, not on Allegheny Airlines.). A model typically is a complex set of theories, constructed to deal with a phenomenon in which every single element or component cannot be probed individuallly. Therefore a model is constructed from what is known, the gaps filled in with hypotheses, and the resulting device used as a framework for predicting outcomes that are experimentally verifiable.

Kleenee set out in the 1940's, probably for the first time, and probably better than anyone since, the idea of a model

being justified when we are dealing with something quite complex--to complex to keep track of all of the separate components.

We would not, for example, call Fechner's Law a model; we might in a loose sense, but we certainly wouldn't want to add that level of sophistication to something as seemingly simple as that relationship. Models growing out of theories are very common in the physical and biological sciences. There is, for example, the Ptolomaic model of the universe that is still today a reasonably good approximation to observable phenomenon, and certainly was in its days the best thing going. There is Bohr's model of the atom, and there is the Crick and Watson double helix model for DNA.

All of these have served very useful functions in the history of science, in holding together complexes of theories, filling in a certain amount of hypothesis, and eventually instigating further experiments that lead to changes in the models.

If the model does not predict something we can test, that is, if the model cannot and does not lead to its own destruction, it is probably a useless model. We don't have much use for models of complex phenomena that don't allow us to build improvements on the models.

In reading there are theories and models that deal with the processes of reading, whether in the child or the adult, theories and models that deal with the learning process,

either learning in general or the acquisition of literacy itself, and theories and models that deal with instruction and the instructional environment.

I tend to agree with Gibson and Levin in that we probably don't know enough to build useful models of the total reading process and that we are truly chasing after the wind in doing so. As I look at the current attempts to model the total reading process, with a 'psychclinguistic' box here and a 'morpheme' box there, I think of a comment that Berthold Brecht made many years ago; "When you have everything in the wrong place, that's disorder; when you have nothing in all the right places, that's order." Well, order is what we have in most if not all of the general models for reading.

Where, though, we do seem to gain profitably in experimenting with models of reading, are models of specific and usually quite limited processes in the overall reading process. For example, Hochberg developed (and I hope he has deserted it now) something called "The Guided Eye Movement Model". From his own and from other studies he hypothesized that the eyes while focusing on one area in reading could look quite far ahead in the periphery and interpret enough of the visual information to decide where to focus next. Included in this model were various types of search mechanisms. The work of McConkie and Rayner, however, in showing extreme limitations on what the eye can do with information in the periphery, casts considerable doubt on the validity of this model. Nevertheless, this was an interesting model that lead to interesting experimentation.

In the Smith and Kleiman paper and in the Juola paper we heard rather good discussions of the most sophisticated and useful models we have for reading processes--those for information processing during word recognition.

I should remind all of you that the earliest information processing model for word recognition did not originate with Sperling or Nisser, but with Quantz around 1897. Quantz's model included stage by stage processing, with iconic storage, read-out, and short-term memory components. This model developed from a concern for measuring the speed of various mental events, a concern that was fundamental to experimental psychology at that time. Quantz's model is quite interesting within its own right and quite similar to what we are doing now.

Smith and Kleiman described how a model is built and then used to derive experiments which themselves lead to changes in the model. The authors stressed three problems which need to be resolved and which still require improvements in the model: (1) the problem of units of interpretation, (2) the problem related to lexical access, and (3) the problem of context effects

The utility of models for building better models was further exemplified in the Juola paper with a discussion of word recognition studies which used fixed sets of target letters. The apparent ability of subjects in such paradigms to utilize the features of the target letters to reduce response time

necessitates changes in the model to allow for overt control by the reader of the reading strategy. Reading models require a control mechanism to account for changes in behavior based on the input. The reader (or subject) seems to say in certain reading situations "Let's try a new strategy, let's do something different." and somehow the model has to account for this. In the current information processing models this by itself should not be difficult to achieve.

This same control mechanism can probably be used to account also for what we have been calling inside-out factors. An approach to this was suggested over a decade ago by Broadbent in relation to the frequency effect in word recognition. Broadbent suggested that when we have a high expectation for a particular input, we are willing to make a decision with less visual information than when we don't have such a high expectation for the input stimulus. So in effect we could account for expectation within an information processing model in terms of either shifts in criterion levels, or other shifts in the processing strategy that would allow attention to fewer visual elements.

There are, however, alternatives to the stage by stage models that were presented here. Turvey, for example, hypothesizes a much more complex model, much more neurologically based, in which information in different forms travels at different rates through the system. Thus, gross outlines of figures would be available for matching and decision making before finer visual details would be available. Time varying transmission

will of course also allow a shift in criterion level, as was just suggested for building in a control mechanism.

Turvey, as in many other models including Massaro's, has incorporated parallel processing at an initial stage. Turvey also raises an anomaly that I think needs further attention in discussions of information processing models, and that is that although we think in terms of features like lines and curves building up into wholes, Turvey points out that perception must also include processes that work in the opposite direction. We don't know when we have a straight line or a curve until we have the whole. What, for example, could be a straight line in one type of figure might be a curve in another. So there has to be a buildup from retinal images into a whole, and then the extraction of features from the whole for recognition.

Parallel processing is also used in Turvey's model as it is in Massaro's model, to account for effects like the word effect. I am not sure whether Smith and Kleiman posit parallel processing or not.

Another area which was not discussed in any of the papers was that of developmental models for reading. For information processing approaches the work of Marshall Haith and his students seems particularly relevant. What is exciting about their work, even though it deals with geometric figures and not letters, is that it begins to explore the very early stages of processing and to show where children do and do not differ from adults in visual processing.

One of the results of this work, if the data are being properly interpreted, is that the earliest stages of processing including the building up of an iconic image, the recognition of a single input image, and the ability to recognize images in the periphery, are almost identical for the five year old and the adult. The two begin to differ only when the cognitive load gets heavy and when immediate memory seems to be required. This is quite an oversimplication of what Haith and his students have presented, but one hopes that this line of inquiry can be pursued with letters and words. It seems especially promising for building developmental models of word recognition.

Finally, certain types of experiments that relate to developmental models, but are not within themselves information processing experiments, should be mentioned. In particular, I am thinking of the work of Roberta Golinkof and of Rosinski and Wheeler, both of whom have examined the development of the recognition of orthographic regularity. Their methodology usually involves judgments by children of which of a pair of synthetic words is more like a real English word.

It is clear that an interest in information processing has motivated the most exciting word recognition studies today; however, there is a caution that should be made that I think was best stated in Tennyson's version of the Arthurian legend. Arthur, on his death bed, supposedly says, among various other things, that "The old order changes, yielding place to new, and God fulfills himself in many ways, lest one good custom should corrupt the whole earth."

What I mean is that there is always room for alternative approaches, whether for convergence, or for simply ensuring that a particular explanation is not ignored. Certainly the approach used by Gibson and her colleagues in their original studies of spelling units would be a good alternative.

When we turn to models and theories about learning to read, the air becomes considerably more murky, and what we hear often sounds like noises from a shroud, to borrow from the Ancient Mariner. We have, for example, Piaget's theories, which don't attend directly to reading at all, but have been interpreted by Furth and others to imply that reading is a rather low level skill and that reading pedagogy should center on discovery procedures so that children will find on their own what reading is all about.

However, many reading skills, such as attending to the orientation of letters, are abilities that the child could rarely discover on his or her own. If there is anything the child discovers in initial stages of perceptual learning, it is the invariance of labels for objects with orientation change. A cup is a cup whether the handle goes one way or another. Letters and numbers are the first objects, and probably among the only objects, that the child ever encounters in his early schooling in which orientation makes a difference for labeling. The attempts by Piaget's followers in the USA to induce children to discover these types of relationships, both in the verbal domain and the sound domain, seem to have failed.

On the other hand, some theories about reading must be teased out of practice. For example, the argument between so-called whole word instruction and phonics instruction boils down not to an argument over letter sound learning (since both methods accept it), but instead to an argument over which approach is more effective as an introduction to reading.

The whole word people claim that letter sounds are too abstract and too complex for children to manipulate in their initial encounters with reading. Therefore, starting with whole words is done to build up motivation, interest, and sensitivity to the task so that letter sound learning of one form or another can be introduced later.

The hard core letter-sound people say, on the other hand, "The hell with all of that nonsense; let's just get right in there with the letters and the sounds."

Many programs like Distar carry implicit assumptions about learning to read and, particularly about information loads that children at different stages in their development and from different backgrounds can handle.

In reading instruction are found yet another set of theories, and I am thinking here of theories of people like Stephens, who have a very different view of the teacher's role in instruction than we have heard here today. According to Stephens, the teacher's role is not really to teach very much, but to give

children an idea of what is important, so that in their natural exchanges with other children and with adults outside of the classroom they will focus on those elements that are important and therefore will acquire them.

On the other hand are the suggestions from studies by George Weber and by the New York State Department of Education that the most important variables for success in reading are such matters as instructional organization, teacher attitude and teacher training and not the basic skills of the child, or the particular instructional method that the teacher is using. I think that these studies should be taken seriously. Certainly the Clay paper takes a strong view on where the action in reading should be by stressing teacher training and teacher centered instruction, as opposed to the early CAI views described by Fletcher that wanted the teacher as far away from the child as possible.

Now, having displayed how muddled reading theory is, let me try to make more sense of reading practice. The problem with discussing reading practice is that no one to my knowledge has ever attempted to delineate what is involved in reading instruction. Those who have developed reading programs are aware of the complexities of the instructional task, but most instructional decisions are not made overtly.

We heard some concerns with practice in the Bartlett paper which reviewed two reading programs, in the Chomsky paper, the Liberman paper, in the Fletcher paper, and to some degree in the Calfee paper, at least by implication, with the particular kinds of assessment tests that were talked about.

I'll use letter-sound learning to give an idea of the number and complexity of decisions that must be made in instructional design, and also to give an example of what we would have to attend to if we wanted all classroom practice to derive from theory. In doing this I am assuming that reasons exist for teaching letter-sound correspondences. One of the first decisions that has to be made is how to teach them. Do we use inductive or do we use deductive approaches? Perhaps there is some theory we can draw upon. Certainly everyone agrees that inductive approaches are considerably better than deductive approaches at the early grade levels. But what if we were teaching letter-sound correspondences to adult illiterates? Would we teach them rules or would we not teach them rules? What empirical base would we draw on to make that particular decision?

How many and what types of exemplars would we use in introducing letter-sound patterns? Recent studies on letter-sound learning indicate that this latter question is quite important. For example, children in grades 1 and 2 are introduced to the patterns for the letter c in initial position. In this position, c has a soft sound if followed by e, i or y; otherwise, it has

a hard sound. Children in the lower grades usually do not encounter expections to this rule. (Cello is probably the only word that most people ever see in their lifetimes that is an exception.)

What tends to happen regardless of reading program is that the children don't learn this particular pattern as well as many patterns that are much more complex. They develop a strong response bias towards the hard sound /k/ in early reading and don't completely overcome this bias by the end of letter-sound instruction. As far as we can see from longitudinal studies, adults don't generally do better than 65 to 70 percent correct on the soft pattern when tested with synthetic words.

If we look carefully at what children are trained on when they learned the c pattern we see that in the most popular reading programs about 95 percent of the initial c words that are introduced through third grade have the hard pronunciation. Perhaps three or four words in these readers begin with soft c. Clearly this bias in exemplars has an effect on what is acquired, regardless of whether a rule is verbalized or not.

Beyond the question of exemplars lies another question concerning c and g, and the five vowel letters. Do we introduce the alternate pronunciations concurrently or successively? Levin and Watson raised the possibility of concurrent introduction and Williams and some of her colleagues tried experimentally to compare the two approaches, as did the Cornell Reading Project.

Another question concerns word position. Do we introduce letters for patterns in initial position, because that is the easiest position to attend to, or do we introduce them in other positions so that children will not over attend to beginnings of words?

In the 1930's Hill showed that one of the major effects of early instruction in reading was to shift the child's attention from various parts of the word to the beginning of the word. Reading programs place major emphasis on the initial parts of words and thereby distract the child from attending carefully to the remainder of the word.

Do we use contrast in instruction or don't we? Do we take phonetically similar items, like pet and pat, and present them together, to help the child make discriminations, or do we keep them apart to avoid confusions?

How do we relate letters to sounds? Do we present sounds in isolation, or do we believe Bloomfield and Fries that this is an abomination, and present them only in context?

Do we associate letters directly to sounds, or do we use letter names to mediate between letters and sounds, or do we use objects to mediate? And which prereading skills should we teach before formal reading instruction begins?

The Liberman paper and the Chomsky paper suggested pre-reading exercises that would bring the child's attention to the phonic nature of the spelling system. Doris Johnson's paper,

in discussing learning disabilities and the diagnosis of intrasensory before intersensory abilities, also touches on this particular problem. We have a large literature, including studies by Elkonin, Zhurova, Bruce, and others, that attend in a very deceiving way with this problem, as I will discuss shortly.

Do we use only meaningful words or do we use nonsense materials?

How do we sequence letter-sound correspondences? Do we introduce overtly all of the consonant clusters that exist in English, as does at least one commercial program or do we depend upon transfer to shorten the training period?

And then what kind of assessment do we do? This is a question attended to in part by the Calfee paper. How much assessment do we do? What kinds of assessment instruments do we use? Do we need highly reliable ones with large numbers of items or can we teach teachers to do informal assessment?

It is clear that we cannot, and probably would not want to research all of these questions. As Fletcher pointed out in describing the Stanford CAI project, building a reading program requires making many arbitrary decisions. Where classroom experience is not available, you make the best judgment you can, observe what happens, and make modifications if necessary. It's in a sense trusting that Francis Bacon was correct in saying that truth will emerge more rapidly from error than from chaos.

Whatever might be the difficulties of building sound instructional programs, the task of finding practical implications from reading theory is exceedingly more difficult. The gap between the two is the La Brea tar pit of education and seems to mire many people. We heard, for example in one of the papers given on the first day: "Consider what we know about our writing system, namely that it is alphabetic and not ideographic. From this it would seem to follow that instructional procedure should inform the child early on that the printed words is a model of the components and phonemes, and their particular succession in the spoken word."

Now, to me this is as wrong as you possibly can go. Instructional decisions have to be based on a large number of issues such as the entry level skills of the child, the methods for instruction available and their complexity for teaching, and available resources. It is incorrect to argue that where you are going determines absolutely how you get there. A number of strategies exist for teaching letters and sounds, yet there is no experimental justification for claiming that the best way to teach these is to make anything clear to the child from the beginning of instruction.

Premature leaps from theory to practice represent one problem in this area. A second problem is represented by good experimental evidence that fails to influence practice. Harry Singer has published a paper called "Research That Should Have Made a Difference." In it he mentions four or five studies that attend to issues that are important for reading

instruction, yet little that was discovered in these studies ever got into practice.

For example, he cites one study that demonstrated that choral reading, that is, reading in unison, is almost worthless for beginning readers. The children in the early grades read at different rates and in general cannot adjust their reading rates easily; therefore, they have difficulty reading along with others. Yet we heard that in the Open Court Program in this country and in the teaching of reading in Mexico this practice is still favored.

The history of reading instruction in this country shows a strong similarity between reading practice and religion. Both reading methods and religion tend to be charismatically based. We have in religion, of course, the charismatic individual, the hero with the thousand faces: Moses, Jesus, Peter, Joseph Smith and so. In reading we have the same thing: Parker, Dewey, Thorndike, Gates, Flesch, Pitmann, and the other gods of reading.

Educators justify particular practices on the authority of the particular reading god they worship. And when reading becomes a problem, they throw out one god and bring in a new god. "The king is dead, god save the king." There is truly a belief here in the one best method, which derives I suspect from the American belief in the pastoral dream. There is one best way to teach reading and we must constantly search for this holy grail of education. This seems to be more befitting of Dr. Pangloss than it does of educational research, yet it seems to go on and on and on.

Much of this attitude derives from the role that publishers play. The reading market in the elementary grades alone is rumored to be worth a little over $300 million a year. One publisher supposedly invested over $8 million in developing its latest reading system. No matter what new evidence is revealed about reading instruction, this particular publisher will not be changing its reading program very soon, not with $8 million already invested in it.

It was pointed out the other day that commercial programs hold a major grip on what is used in the classroom, yet the major influences on publishers is not just pedagogical concerns. For example, every reading program on the market is developed to be acceptable in Texas, because every publisher can break even by making the Texas adoption list. What Texas wants to see in a reading program has a high chance of appearing.

Finally I want to return for a moment to the studies by Elkonin, Zhurova, Bruce, and other to discuss the difference between what we can learn from a laboratory setting and what we have to learn from a classroom setting. Elkonin, Zhurova, Bruce and various others proved conclusively that children before some magic age, six in Bruce's study, seven in Zhurova's and so on, could not perform certain tasks that involve manipulating sounds as abstractions. The children couldn't segment words, they couldn't pull the first sound off, and so on and so forth. Very convincing evidence from every nice studies.

However, most of these studies involved 15 or 20 training trials of some small set of stimuli, and 15 or 20 transfer trials. And from that evidence the authors generalized whole theories about reading, or about sound segmentation, or about the development of certain abilities in children.

The great shock comes when you take exactly these tasks, or slight modifications of them, into an ongoing classroom with the same age children and start doing these things day after day. You soon discover that almost all kids from kindergarten up can be taught all of these tasks without tears or frustration. All it takes is repeated practice with appropriate training. There is a world of difference between the ongoing classroom and the laboratory. One of the most pressing needs today in reading research is improved methodologies for experimentation in the classroom.

Perhaps one brief example will summarize these last few points. The role of letter names in reading instruction is problematic. We heard references to letter names in instruction in the Fletcher paper, the Liberman and Shankweiler paper, and the Chomsky paper. Fletcher noted some well-known correlational studies that show that letter names are good predictors of reading success, but yet also stated that the experimental attempts to show facilitation by letter naming of any reading task have not been successful. Based on this review of the literature, no direct attempts to teach letter names were made in the 1500 curriculum, but letter-name instruction mysteriously appeared on the PDP-10 curriculum.

Liberman and Shankweiler said on the other hand that we should begin reading instruction, as many so-called phonics programs do, by teaching the child to associate the shape of the letter with its name and the sound it makes. There appears to be quite a lot of empirical data to attend to this issue, yet none really answers the question in a satisfactory way. We have data on the use of labeling in discrimination of objects by adults, using geometric patterns, fingerprints, and so on. We also have the studies that Fletcher pointed out on the effects of letter names on word and letter discrimination.

The main advocate of using letter names for mediators in letter-sound learning is Durrell. But Durrell seems to have failed to look at the alphabet in relation to the names of the letters. Three of the letters (w, h and y) don't even contain the sounds that they are suppose to mediate for. Seven others (the five vowels, c, and g) contain the sound that is typically taught second in reading programs today. Of the others, seven are made up of vowel-consonant combinations and the rest are made up of consonant-vowel combinations.

In addition, anecdotal evidence from Russia, Israel, and the United States indicates that confusions often arise when the letter name is stressed along with the letter sound. The child often perseverates on the name and uses it to respond to the letter even when the sound is sought.

There are other problems in relating theory to practice, but perhaps its time to conclude by asking where should we go from here. The first direction I think we have to take is to refocus attention on the school, the classroom and the teacher. We have to begin by defining problems that exist at these levels and then work back to the laboratory. That is the first step.

Second, it's clear, as Conant stated years ago, that a revolution in teacher training is needed. Courses on reading methodology in colleges and universities do not prepare teachers for making their own instructional decisions. At best these courses prepare teachers for locating and following the teacher's guide in a published program. We also need to develop efficient in-service training methods and as was mentioned earlier by Jerry Rosner, we need a better dissemination network. This country seems to have so much money for education that 5,000 groups of teachers around the country every summer can sit down and reinvent objectives for kindergarten through twelfth grade reading, without any one knowing what the other is doing.

In every city where I have examined reading programs, groups of teachers are working independently to develop reading objectives. Perhaps it would be helpful to circulate some of these.

Then, I think we should follow-up both the studies of school organization, such as the Weber study and the New York State study, and a study done by Barton and Wilder for the Carnegie Corporation a number of years ago on the training of the reading experts. This study examined, among other things, who the reading experts were, how much they published, and what they published.

Then I think we need a new science of experimentation in the classroom. Piaget hinted 35 years ago how this might be done with what he called "experimental pedagogy." Ongoing classroom programs would be examined, marginal changes made in the instructional methods, and measurements done of marginal gain or loss. This process would be repeated with other program components to build up an understanding of how instruction works.

At the same time, however, I advocate that we continue basic research. Good researchers must be encouraged to pursue problems related to human processing of any kind, to provide the fundamental information that is essential for understanding any complex phenomenon.

We need, though, to improve the communication among researchers. We have now in the word recognition area something that approaches the high level of communication that is found in Watson's account of the development of the DNA model and in the literature from the turn of the century on the experimental study of reading.

At that time communication seemed important. The laboratories where work was going on were well identified, researchers visited back and forth and referred to each other in polite tones, as was the tendency in the literature of that period.

It would be a pleasure to see this going on again in reading research, rather than seeing, as we see outside of the word perception area, a myriad of isolated researchers, scattered in cluttered obscurity around North America, doing very often the same things with almost no communication among them.

OPEN DISCUSSION OF VENEZKY PRESENTATION

E. SMITH: You mentioned that we should have alternative types of information-processing models. You mentioned Turvey as an example. That is a good example; he takes a much more physiological point of view. You mentioned Gibson as an alternative style of modeling. Could you tell me what that style of modeling is?

VENEZKY: No. Let's just say that she has an alternative approach to investigating the world. Now, how she actually calls that a model, I don't really know. But I would offer that as an alternative approach.

CALFEE: The Gibson-Levin book, I think, presents a representation of a way of thinking that is very different from information processing for reading. It is hard to characterize the difference, because information-processing models, themselves, are not always a well defined class of models.

GLASER: What sort of framework would you impose upon your important direction of working back from the classroom? Would you work from the classroom to investigation? That's been going on, and the journals are full of investigations of teacher practices in the classroom. What kind of framework would you impose on it, to have it go in the directions you would like it to go in?

VENEZKY: We could have had exactly this going on over the last three years with NIE, but NIE chose to go in exactly the opposite direction. They asked people like the researchers here to sit down and tell them what they would like to do with NIE's money. They asked for suggestions that might, in some way relate to

reading, so they could develop priorities for reading research. If you read now, for example, in the new guidebook for submission of programs, you find that eye movements are a very big part of the NIE skill priority. Now, damn it, tell me where in the world we have a problem in reading instruction related to eye movements. But there are people in NIE who feel we should spend more time with eye movement cameras. Therefore, there is going to be a lot of money put into fancier studies in eye movements. Supposedly, these studies will suggest ways to improve reading instruction. Now, I will grant that there is a good place for eye movement studies. But to call that research related to reading instruction is nonsense.

If NIE, rather than bringing together those of us who were tethered to that damn motel at Dulles Airport two or three years ago, had brought in teachers and reading specialists and given them a little better food and a little more hospitable environment, we would have had some very good priorities for research, derived directly from the kinds of problems that are going on in the classroom. I am sure people here today could tell us about things that are both immediate and long-range problems.

BARTLETT: But, Dick, NIE does have a section, in the new guidebook, on teacher interaction in the classroom. I think that is significant.

VENEZKY: Granted, there are some things there, but, once again, even the teacher conference had a very heavy emphasis on the researchers' views of what to do.

I realize I am being a little extreme here, but when you ask for a framework, clearly, we could use ongoing mechanisms. We, clearly, could use NIE's conferencing priority setting mechanisms to identify classroom problems.

Those of you who go around to schools to meet with school people, specialists in reading and language arts--I don't think there is any doubt in your minds about what some of the important problems are. Certainly assessment is a major problem in schools today. The Calfee paper attended very well to the kinds of things that are problems and some of the directions that seem to be needed. That is the framework, in fact, that could be used.

GREGG: I have no quarrel with almost everything that you were dealing with this morning. I notice, though, that you, too, are focusing on a special part of the reading problem, namely the early instruction. I think that the NIE guidebook that you mentioned is talking about reading comprehension, perhaps, at a very advanced level. I would just like to point out that we have, in our laboratory, some data that shows that the peripheral vision is very powerful in picking up visual regressions, regressive eye movements, that may tell us something about where problems of comprehension occur. Once again, it's basic research, and not very directly related to the practice of instruction as such.

About the models that you were talking about: None of us has really come to grips with statements about very specific instructional tasks. Glaser just said LRDC does, but remember that at the end of your talk, you were talking about very specific decisions that the teacher had to make in the classroom. None of the theoretical models that we formulate in the psych lab are patterned directly after those specific needs of the teachers.

I think a strategy for information-processing models involves picking a small task, a particular task, and looking at it very hard, trying to get at those timing parameters and the way the components work together. Unfortunately, in our place, over at Carnegie-Mellon, this has led to a lot of work on chess.

If we just wanted to teach chess, or if the subject of this conference was just chess, we could say a lot. Perhaps, if someone like LRDC would start zeroing in on some of the specific kinds of teacher problems, or if we could get some of them today from the teachers, themselves, to add to our list, we could come up with the kinds of things that I think the information-processing models can deal most directly with. Isabel Beck and Resnick have one on blending and the grapheme-phoneme correspondence. That's a good start in those directions. So more of those, perhaps, are really called for.

VENEZKY: Lee, your last point requires me to compliment NIE for giving the award of their teaching research center to Lee Shulman's group, which places a very strong emphasis on the work to be done on the teacher as a decision maker, an information processor. I know Lee's work, in many years, has gone in that direction, generally with hospital staff people. What he has learned, fundamentally, I think, is going to be applied profitably to reading decisions.

BARTLETT: I think the notion of educating doctors fits in with the notion of educating teachers. Doctors have to diagnose what's going on with patients and make prescriptive decisions on the spot. If a model like that could be incorporated into our teacher education process, if we even knew how to design such a curriculum, or at least how to begin to work on such a curriculum for teachers in education institutions, we might begin to get at the kinds of problems that we obviously have.

VENEZKY: That's a good contrast.

CALFEE: There were three teachers, or three people associated with the teaching

profession, at Dulles. Mike Smith's wife, Nicki, Kate Hoover, and a representative of the National Education Association were all there as informal observers. Shirley, that's the reason people who raise questions like yours ought to sit around in conferences like this. Those three people were not able to do that in any effective way. They could not keep raising questions such as: What are you talking about? Will you put it in language that other people, users, might be able to understand? I think translating research findings into language that practitioners can understand is an important exercise for the researchers. Although you have to take the advice with a grain of salt, it is good for you to be asking, because you might get a useful answer, and it is good for researchers to worry about that. It is often tortuous, especially if you are in the minority, which is very often the case when researchers set up conferences of this sort. But I think we are going to have to keep working on it. I would dearly love to see NIE or some organization take a more active role in finding good mechanisms for running conferences of this sort, so that a variety of points of view can be effected or accepted.

MARTUS: In part, it seems to me a political question, political in the sense of what part people like yourselves can play in experimenting with ways to communicate with teachers in their own setting. There is, in fact, no support system for teachers in most public schools. It should be part of the school system's official responsibility to have places where teachers can turn for help with questions, with issues that come up in the course of their daily practice. We look at what's there in the school system to help teachers learn from their experience and find that the system not functional in that sense.

VENEZKY: Why don't you say something about the teacher center work that you have

been involved with.

MARTUS: The teacher centers have arisen, as some of you may know, in an indigenous, grass roots way. People come because they are desperate, because they are interested, because there are some other people they want to talk with, or because someone is going to do a workshop that relates to something they are working on. In about 30 places around the country, teacher centers have attracted 2,000 or 3,000 teachers, who come regularly or irregularly on their own time to try to get help. When you find yourselves saying, "Gee, well, teachers don't really have the latest word. They don't know what research is going on in my area. Isn't it a pity," Consider those teacher centers as places to experiment with ways of communicating across disciplines and across roles.

END SESSION

Saturday Afternoon, May 22, 1976

DISCUSSION BY SHELDON WHITE

RESNICK: Sheldon White is our final discussant.